

## BIOLOGICAL NOTES

MUSEUM PESTS IN ENTOMOLOGICAL COLLECTIONS.—Every record of successful contests with destroying agencies in museums will be of interest to collectors, and useful hints as well as valuable data in natural history may be derived from accounts of the animal species which appear in museums in widely separated localities. Very little information is as yet in print concerning museum pests in America, and Dr. H. A. Hagen has rendered a service to science by relating his experiences in the entomological collection of the Museum of Comparative Zoology, Cambridge, U.S. (*Proc. Boston Soc. Nat. Hist.*, 1878, p. 56). He found *Dermestes lardarius* very abundant at first, but as it was easily recognisable it was soon extirpated. The excrements of the larva are large, granulous, and jet black. A much more dangerous foe is the larva of *Attagenus megatoma* (family Dermestidae), having small globular ochreous excrements. Dr. Hagen was always able to find the crack in the box, through which the very thin and slender young larva entered. Anthrenus (same family) is represented by three species, of which the commonest in Europe (*A. museorum*) is very rare in Cambridge. *A. varius*, rare in Europe, is the most common pest in the collection, especially in new additions. *A. scrophulariae*, the "carpet bug," has only been known to Dr. Hagen since 1872 in New England, and later still as a museum pest. It has since become very abundant and dangerous. The excrements consist of very fine light brown globules. *Ptinus fur*, so common in Europe, very seldom attacks the insects at Cambridge. *Tribolium ferrugineum* was imported from the East Indies. The flat body of the larva, as well as of the beetle, made it particularly fit to enter boxes through the smallest crack. It was got under by the aid of tobacco-smoke. The common clothes-moth was found very dangerous, and taught Dr. Hagen the lesson of placing every spreading-board at once in a tightly-closing box. Several years ago Dr. Hagen inclosed some clothes-moths with rotten insects in a glass-stoppered bottle, where they propagated year after year. In the fourth year the moths were noticed to be visibly smaller, and in the sixth most of them were scarcely half the ordinary size. But among them were always one or two of the normal stature. Nothing had been added to their food in these years. Two injurious species of *Psocus*, which are much attracted by paste and glue, do not infest Dr. Hagen's boxes, which are not lined with paper. Further, every spreading-board is carefully cleaned, and the cracks washed with alcohol before use. Acari have been easily kept out. Dr. Hagen strongly urges the entire separation of entomological rooms from those containing other preserved animals, especially mammals, birds, and skeletons. Having well-closing boxes, and never putting anything into the collection before it is safe, are the only satisfactory precautions. All other recommendations are but a poor substitute for bad boxes.

EFFECT OF LIGHT ON PELOMYXA.—In a very low, amoeba-like organism, *Pelomyxa palustris*, Herr Engelmann recently observed a remarkable action from sudden incidence of a moderate light (*Pflüger's Archiv für Physiologie*, Bd. xix. p. 1). Watched in the microscope this organism showed very slow movements, which, however, on shading the object, became much more lively. When the hand was removed, the granular mass in the interior became still, and the body contracted into a ball, as after an electric shock; this effect occurred within a few seconds. With continued moderate light, weak changes of form appeared again, with hardly perceptible locomotion. This experiment was several times repeated with equal success, and the results were especially notable in a dark room, into which diffuse daylight could be admitted. When, however, the room was illuminated, not suddenly, but gradually, the pelomyxa showed no effect.

THE OVULE.—One of the most important of the recent contributions to the embryology of plants is, without question, Prof. Warming's memoir, "De l'Ovule" (*Annales des Scien. Nat.* six<sup>e</sup> sér., Bot., tome v.). Commencing with a sketch of the views as to the origin of the ovule and nucleus, held in 1844 by Ad. Brongniart, which is in brief, "that there are two different origins for the ovule, the one to be found in the immense majority of flowering plants, in which the ovules make their appearance on the edges of the carpellary leaves, and represent the lobes or dentations of these leaves, the other confined to a very small number of plants (Primulaceæ, Myrsinaceæ, Theophrastæ, and probably the Santalaceæ), in which the ovules correspond to so many distinct leaves carried on the prolongation of the floral axis. The nucleus is a production *de novo*, a cellular mamelon developed on the upper face of the lobe of the leaf and in the bottom of the cavity where it is formed." Cramer admits and confirms this view. Van Tieghem and Celákovsky also agree to it, with this difference, that to them all the ovules appear as lobes of leaves, and that there are no independent ovular leaves. Warming, too, admits the theory. In every ovule there are, however, two parts essentially different, the funiculus and the teguments, which are, without doubt, of the nature of leaves, and the nucleus, which is a "new" creation—a veritable "sporangium," as Prantl says, a "sorus" composed of a single sporangium. Discarding for a moment the question whether ovular leaves exist or not (for example, in the yew), Warming doubts whether the funiculus should always be considered as foliar in certain cases. It is, perhaps, only a metablastome sprung from a leaf, but what is essential is that it is never a bud. In the memoir Warming looks at the question steadily from the point of view of histogenesis. The memoir consists of three chapters. On the early development of the leaf or ovular mamelon; on the genesis of the nucleus; and on the formation of the integuments of the mamelon. These chapters present a most masterly review of the whole question, one in which every due credit is given to the past and present workers in this field of research, and to none more than to the ingenious Slav botanist, Ladislao Celákovsky. It is somewhat difficult to epitomise the author's conclusions, but he recognises that the morphological significance of an organ does not absolutely depend on its position. He demonstrates that the theory of Brongniart is the true and solely admissible one, and he reasons very conclusively against the views of Bronn, Eichler, and Strasburger, who would regard the ovule as a bud, while, in reality, as he says, "the ovule is the homologue of a sporangium."

MUSCLES OF CRAYFISH.—While many observations of muscular contraction in vertebrate animals (especially the frog) are on record, the muscles of invertebrate animals have been little studied. M. Richet has recently directed his attention to these latter. From experiments with the myograph on the muscles of crayfish, he finds that the contraction of the muscle of a claw lasts nearly ten times as long as that of the caudal muscle (80 to 100 stimulations per second were needed to cause tetanus in the tail, while 2 to 4 sufficed for the claw). The contraction was not more retarded in one muscle than the other; with strong direct excitations, it is about a hundredth of a second; with weak it may be four or five hundredths. With the former stimulations applied to the ganglionic chain instead of directly, the total retardation is about 2½ hundredths of a second. The muscle of the tail is very quickly exhausted, and this agrees with the fact that crayfish cannot swim any great distances. On the other hand, the muscle of a claw, stimulated by very closely following electric currents, is not exhausted. While the tetanus of the caudal muscle does not last more than twenty to thirty seconds, the muscle of the claw remains contracted nearly half an hour, and during the first five minutes the constriction of

the claw is stronger and stronger. This, too, accords with the habits of the crayfish, which will almost sooner die than let go its prey when seized. Thus between the two chief muscles of the crayfish there is a difference at least as considerable as between the striated and non-striated muscles of vertebrates.

### SUSPENDED ANIMATION

THE statements in the *Times* of Monday, which, under the head of "A Wonderful Discovery," are copied from the *Brisbane Courier*, seem greatly to have astonished the reading public. To what extent the statements are true or untrue it is impossible to say. The whole may be a cleverly-written fiction, and certain of the words and names used seem, according to some readers, to suggest that view; but be this so or not, I wish to indicate that some part, at all events, of what is stated might be true, and is certainly within the range of possibility.

At once let me state that the discovery, so called, which is described in the communication under notice, is not in principle new. On the subject of suspension of animation I have myself been making experimental inquiries for twenty-five years at least, and have communicated to the scientific world many essays, lectures, and demonstrations relating to it. I have twice read papers bearing on this inquiry to the Royal Society, once to the British Association for the Advancement of Science, two or three times in my lectures on Experimental and Practical Medicine, and published one in *NATURE*. In respect to the particular point of the preservation of animal bodies for food, I dwelt on this topic in the lectures delivered before the Society of Arts in April and May of last year, 1878, explaining very definitely that the course of research in the direction of preservation must ultimately lead to a process by which we should keep the structures of animals in a form of suspended molecular life.

Let me next point out what, by experiment, is known as to the possibility of suspending animal life.

If an animal perfectly free from disease be subjected to the action of some chemical agents or physical agencies which have the property of reducing to the extremest limit the motor forces of the body, the muscular irritability, and the nervous stimulus to muscular action, and if the suspension of the muscular irritability and of the nervous excitation be made at once and equally, the body even of a warm-blooded animal may be brought down to a condition so closely resembling death, that the most careful examination may fail to detect any signs of life. I have shown in a Croonian lecture that there are three degrees of muscular irritability to which I have given the names of active efficient, passive efficient, and negative. The first of these states is represented in the ordinary living muscle in which the heart is working at full tension, and all parts of the body are thoroughly supplied with blood, with perfection of consciousness in waking hours, and, in a word, full life. The second of these states is represented in suspended animation, in which the heart is working regularly, but at low tension, supplying the muscles and other parts with sufficient blood to sustain the molecular life, but no more. The third of these states is represented when there is no motion whatever of blood through the body, as in an animal entirely frozen.

The second stage, the stage of passive efficiency, is that in which animation is usually suspended. The condition is a close semblance to the third stage, but differs from it in that under favouring circumstances the whole of the phenomena of the active efficient stage may be perfectly resumed, the heart suddenly enlarging in volume, from its filling with blood, and reanimating the whole organism by the force of its renewed stroke, in full tension.

So far as we have yet proceeded, the whole phenomena of restoration from death are accomplished during this

stage. To those who are not accustomed to see them, they are no doubt very wonderful, looking like veritable restorations from death. They surprise even medical men the first time they are witnessed by them.

At the meeting of the British Medical Association at Leeds, a member of the Association was showing to a large audience the action of nitrous oxide gas, using a rabbit as the subject of his demonstration. The animal was removed from the narcotising chamber a little too late, for it had ceased to breathe, and it was placed on the table, to all appearances dead. At this stage I went to the table, and by use of a small pair of double-acting bellows restored respiration. In about four minutes there was revival of active irritability in the abdominal muscles, and two minutes later the animal leaped again into life, as if it had merely been asleep. There was nothing remarkable in the fact, but it excited, even in so cultivated an audience as was then present, the liveliest surprise.

The time during which an animal body may be capable of re-animation from the state of passive efficiency depends altogether on one circumstance, viz., whether the blood, the muscular fluid, and the nervous fluid remain, in a condition which I have defined in another essay as the aqueous condition, or whether these fluids have become pectous. If the fluids remain in the aqueous state, the period during which life may be restored is left undefined. It may be a very long period, including weeks, and possibly months, granting that decomposition of the tissues is not established, and even after a limited process of decomposition, there may be renewal of life in cold-blooded animals. But if pectous change begins in any one of the structures I have named, it extends like a crystallisation quickly through all the structures, and thereupon recovery is impossible, for the change in one of the parts is sufficient to prevent the restoration of all. Thus the heart may be beating, but the blood being pectous it beats in vain; or the heart may beat and the blood may flow, but the voluntary muscles being pectous, the beating is in vain; or the heart may beat, the blood may flow, and the muscles may remain in the aqueous condition, but the nerves being pectous the circulating action is in vain; or sometimes the heart may come to rest and the other parts may remain susceptible, but the motion of the heart and blood not being present to quicken them into activity, their life is in vain.

The problem physiologically before us is as follows:—Can the second or passive efficient stage of life be by any artificial methods secured, so that all the vital parts may be held in suspended animation, working at the lowest possible expenditure of vital power?

Experimental research and experience alike show the certain possibility of temporarily producing this state. Both show that there are agents and agencies by which life may be reduced to the low ebb necessary for suspension of active life, and at the same time the aqueous conditions of the colloidal fluids may be maintained. Cold is the first and most efficient of these agencies. The blood and the colloidal animal fluids derived from it are all held in the aqueous condition of colloidal matter by exposure to cold at freezing-point. At this same point all vital acts, excepting, perhaps, the motion of the heart, may be temporarily arrested in an animal, and then some animals may continue apparently dead for long intervals of time, and may yet return to life under conditions favourable to recovery.

In one of my lectures on death from cold, which I delivered in the winter session of 1867, some fish, which, during a hard frost, had been frozen in a tank at Newcastle-on-Tyne, were sent up to me by rail. They were produced in the completely frozen state at the lecture, and by careful thawing many of them were restored to perfect life. At my Croonian lecture on muscular irritability after systemic death, a similar fact was illustrated from frogs.